

May 13, 1953

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Dear Phil:

Yours of the 11th just received, for which thanks along with the same for letter and report of the 6th. I hope you will have enjoyed your trip to Ann Arbor, (or was it Lansing).

I know just how you feel anent "invariably something I forgot to tell you". There are so many minutiae in this work and our correspondence that it is very easy to miss the forest for the trees.

Has SW-1041 been typed yet (v. my letter of the 6th)? This was *S. gallinarum* --x SW-1040 (IX XII a:--), and appears to show a g... antigen in *gallinarum*. I am sending a group of similar transductions, all --x SW-1040, all g... and derived from the *S. gallinarum* strains as indicated. [When I say g..., I really mean reacting with gm serum; they might be m....] The somatic antigen here is of no special consequence; the point of doing this series was to see whether a variety of *S. gallinarum* strains all behaved alike.

<u>Strain being sent</u>	<u>From <i>S. gallinarum</i> --x SW-1040/a serum</u>
(1043-G1)	#74 (alreadysent as SW-1041) CDC:
1043-G2	2923-49
-G3	2927-51
-G4	309 453 30953
-G5	3728-52
-G7	3968-52
-G8	4614-52
-G10	5522-52

This makes 8 out of 10 *S. gallinarum* tested. The other two (3966-52 and 5285-52) have given nothing in two runs. I am looking into possible trivial reasons for this, but meanwhile do you know anything about these two that would set them off from the others? None of 10 *S. pullorum* strains have given anything in a similar trial.

Now to java. I will tell you my misgivings about what I called "N97" heretofore. I brought N97 ph 1 up with me, and inoculated the unpurified culture into b serum agar, and promptly recovered a 1,2 phase. (~~SW-100~~ At about the same time, I accidentally chucked out the original N97. In hopes of recovering it, as I thought, I put the unpurified 1,2 phase in 1,2 serum and got back what I will now call SW-1007, and had hitherto called "N97" (in quotes). Meanwhile, I had also put a single colony isolate of the original N97b into b agar and got, again, a 1,2 phase I will call SW-1009. After purification, SW1009 also gives a h phase

(somewhat to my surprise), which I will call SW-1009b. Unlike the original N97, both SW-1007 and SW-1009b have given only z33 phases on further selection in b serum. It is apparent that SW-1007 was not a recovery of the original N97b, but that it is a product of the intervening 1,2 phase.

A fresh subc. of N97 arrived recently. Each of 6 single colony isolates gave 1,2 phases after 24-36 hours in b serum agar. However, these 1,2's are not all alike: some will engender a b phase again (like SW-1009); others are stably 1,2:--. [I am just now looking to see whether this difference is already inherent in the single colony isolates from the original N97, or whether different 1,2 phases from the same recently ~~xxx~~ reisolated subculture will behave differently]. ~~There are~~ where it has been possible to go from b -- 1,2 -- b, the tertiary b phase has been stable, giving only z33.. There is something funny about this; N97 would probably not have been described as monophasic if it had behaved this way before, but the time required to give new phases seems to be highly variable.

There are two anomalies about the java derivatives. One is almost familiar, that the 1,2 antigen behaves as a phase-1 homologue. This has been tested, however, only for #157 and for SW-1009. Other paraB second phases will have to be reexamined more closely (and in this connection, I would not at all mind having the type java. I do not think I want to spend more time on further isolates from the same outbreak, unless they have patently different behavior).

The second anomaly is the production of ~~xxx~~ i:b phases. This has ensued from ~~TM--x~~ SW-1007 and ~~TM--x~~ SW-1009b. ~~TM--x~~ N25b gave (for a change as expected) i:-- , and ~~TM--x~~ N97b gave an i:1,2 [further reversibility and homology of 1,2 not yet tested]. In the ~~vert-~~ phase sequence $b_1: 1,2 : b_2$, therefore, it has been the 1,2 and the b_2 steps that have given all of the peculiar results. The original b_1 has behaved like any other monophasic phase 1 [except for generating the others].

I am not sure which batch of cultures it is for which you lack the pedigrees. My letter of April 30 gives the background of SW674B, SW-930, SW-1005, SW-1036 and ~~SW--~~ Here I see what may be missing. ~~SW-1039~~ SW-1039 and SW-1040 are IX XII b:-- and a:-- from S. typhi H901 x-- SW-666 and X-- S. sendai, respectively (see table 1, our ms.: the numbers may be missing. SW-1038 belongs in table 3, IX XII b:1,5 from S. abony --x miami.

SW-1031 has been carried to a:b:a:x (not yet tested), unlike its parent SW-1026 which went only i:b:-(or z33).

To turn to the report dated 5/5/53. I can't imagine what happened to SW-1023. It was inagglutinable in 1,5 and in polyvalent even after a passage in semi-solid. Now it is coming down cleanly in 1,5 and I have the other phase out which will undoubtedly be a, as your report. I really don't know what to make of it, but at any rate no new principle is set up (or broken down).

SW-999B is rather more enigmatic. Its somatic antigen (IV from your report) suggests no possible contaminant; I am forced* to accept its origin from SW999 (IV V XII --:z6 from S. zega --x Hines VAH). I note that SW-998 is given as IV V XII a:1,5. Perhaps there has been a mixup here. I will see if SW-999B can be reproduced from SW-999 and check some of the more obvious alternative possibilities

There is no way out for SW-1003, in spite of its IV V XII. I will send you a number of other a phases that have come up in the course of transduction experiments together with the parent abortus-equi if you want to check this further. SW-1003 is written as TM --x #26, but no TM cells are present, and if there were, how could they be a:enx? SW-1003 resembles #26 closely in a characteristically slow fermentation of galactose on EMB plates. There ~~must~~ be a number of other possible biochemical tests. The TM parent was the wild type. You can use SW-698 or 699 meanwhile; I'll send TM2 shortly.

*maybe not
I'll see.

These cultures are: SW-726 (your #26, passed through semisolid agar), and some new derivatives of the Meyer strains, recently received from you:

SW-1033, Meyer, passed through semisolid as used in these expts.

1042A1. " passed through enx. (1/3 tubes after several days

1042A2 " +TM phage " " 1/3

1042A3.1)

.2) " + TM phage " " 3/3

.3)

The reversibility of these a phases is still being tested. So far, only A2 has given enx.

As in previous experiments, it is not ~~obvious~~ obvious whether the phage plays any role. I will have to go back to #26, which never gave anything by itself. Unfortunately it swarms through my agar rather more slowly than the others.

The other items on your report are more encouraging. I assume x-1,5 is a typo for c-1,5 (SW-1012). I don't know what to make of SW-1021: it may be another artificial phase from miami rather than a transduction of 1,7. I have another experiment running now, S. aitendorf c:1,7 --x SW-1022 a:enx which I hope may give the a:1,7 combination without this complication. I have no z serum, and in my preliminary test of SW-1021 could only verify that it had 1 but not 2.

It is curious that we should be at odds so about SW-986 and SW-674, for I find a confusion of phases in the former but not the latter, and vice versa. I will be content to put this down to the imperfection of my own methods, until something new comes up on the subject. Have you tested the 1,2 phase of the parent of SW-674 (SW-435, which I believe you have) for reaction with g...?

It is curious that SW-986 would swarm in n but not in enx (nor in eh, in my hands). Is S. abortus-equi more patently variable in n serum? What is n (how separated from x, z15....)?

I am sure not to have covered everything, but this barrage may be enough till I get your comments on the ms. Meanwhile, I'll see whether I can straighten out SW-999B. I think the other reports have to be read at face value.

Sincerely,


Joshua Lederberg